

Reviews For Paper**Track** Research**Paper ID** 1298**Title** Locally Boosted Graph Aggregation for Community Detection**Masked Reviewer ID:** Assigned_Reviewer_1**Review:**

Question	
How would you rate the novelty of the problem solved in this paper?	A well established problem
How would you rate the technical ideas and development in this paper?	Substantial improvement over state-of-the-art methods
How would you rate the empirical study conducted in this paper?	Acceptable, but there is room for improvement
Repeatability: are the data sets used publicly available (and thus the experiments may be repeated by a third party)?	Yes
How would you rate the quality of presentation?	Well-written but has a significant number of typos and/or grammatical errors. Would need significant editing before publication.
Which topic category do you think this paper belongs to?	Graph mining
What is your overall recommendation?	Weak reject. I vote for rejecting it, although would not be upset if it were accepted.
List up to 3 particular strengths of the paper. If none, just say "none".	<ol style="list-style-type: none"> 1. A novel machine learning approach for clustering 2. interesting convergence analysis 3. Reasonable experimental validation on the proposed approach

List up to 3 particular weaknesses of this paper. If none, just say "none"..	<p>1. The experimental study does not compare with the state-of-the-art clustering approaches</p> <p>2. Lack detailed related works on the existing community detection/graph clustering</p>
Detailed comments for the authors; justification of your overall rating	<p>In this paper, the authors propose an interesting LBGA approach for graph clustering and demonstrate its results through experimental study. They also provide a formal analysis of the convergence of the proposed algorithm. Overall, I found this is a rather interesting work.</p> <p>However, my main concern is the current work almost completely omits the comparison with the existing studies on community detection/graph clustering. I strongly suggest the authors should consider to add these components before its publication.</p>

Masked Reviewer ID: Assigned_Reviewer_2

Review:

Question	
How would you rate the novelty of the problem solved in this paper?	A minor variation of some well studied problems
How would you rate the technical ideas and development in this paper?	The technical development is incremental without fundamental contributions
How would you rate the empirical study conducted in this paper?	Not thorough, or even faulty
Repeatability: are the data sets used publicly available (and thus the experiments may be repeated by a third party)?	Partially (e.g., some of the used datasets are proprietary)
How would you rate the quality of presentation?	Well-written but has a significant number of typos and/or grammatical errors. Would need significant editing before publication.
Which topic category do you think this paper belongs to?	Graph mining
What is your overall recommendation?	Weak reject. I vote for rejecting it, although would not be upset if it were accepted.

List up to 3 particular strengths of the paper. If none, just say "none".	<ul style="list-style-type: none"> - The proposed problem, graph aggregation for clustering, is somewhat novel. - The idea is simple. (This could be also a weakness.) - The method seems to produce a better representation of graph in terms of clustering.
List up to 3 particular weaknesses of this paper. If none, just say "none"..	<ul style="list-style-type: none"> - There is no complexity analysis and no experiments on the execution time. This kind of EM process usually takes long. - There are additional rating parameters in the method which would take even longer to produce the optimized results if there is no strategical way to tune them. - There is no quantitative evaluation of the method. The baseline is too naive. It must be something more sophisticated than the simple union of graphs.
Detailed comments for the authors; justification of your overall rating	<ul style="list-style-type: none"> - The paper proposes a method for the graph aggregation optimized for community detection. The method is similar to the EM process in that the weights are updated based on the clustering results at each round. The process is proven to be converged. This kind of EM process typically improves the results as it iterates. But its processing time is substantially prolonged. - The idea is simple and reasonable, but this simple idea is questionable if it is technically significant enough to be published in KDD. Runtime and complexity analyses are needed. In Section 3.3, not sure how the fixing edges dramatically speeds up. The efficiency issue must be addressed in more details.
List of typos, grammatical errors and/or concrete suggestions to improve presentation	<ul style="list-style-type: none"> - In Section 3.3., the second line: LGBA -> LBGA

Masked Reviewer ID: Assigned_Reviewer_3

Review:

Question	
How would you rate the novelty of the problem solved in this paper?	A minor variation of some well studied problems
How would you rate the technical ideas and development in this paper?	The technical development is incremental without fundamental contributions
How would you rate the empirical	

study conducted in this paper?	Not thorough, or even faulty
Repeatability: are the data sets used publicly available (and thus the experiments may be repeated by a third party)?	Yes
How would you rate the quality of presentation?	A very well-written paper, a pleasure to read. No obvious flaws.
Which topic category do you think this paper belongs to?	Community detection
What is your overall recommendation?	Reject. Clearly below the standards for the conference.
List up to 3 particular strengths of the paper. If none, just say "none".	<ol style="list-style-type: none"> 1. Well written. 2. Experiments with publicly available datasets
List up to 3 particular weaknesses of this paper. If none, just say "none"..	<ol style="list-style-type: none"> 1. The method is heuristic, without performance guarantees. 2. The experiments do not provide enough evidence that the approach would not induce community structure where it doesn't exist. 3. No comparison with existing approaches.
	<p>This paper deals with the problem of aggregating different graphs on the same node set, each corresponding to a different facet of the relationships, into a single graph whose features are boosted to facilitate a specific graph mining task. The method is illustrated with the goal of find dense graph clusters. The algorithm is inspired by the theory of boosting and bandit learning, and it is demonstrated using synthetic and publicly available datasets.</p> <p>The problem is interesting, challenging, and relevant to KDD, and the presentation quality is high. My major concern for this type of proposal is that I am not convinced whether we are producing a representation of the real phenomena underlying the graph structure or distorting the data or amplifying noise so that we can see what we wish to see there. For example, in a graph with no community structure, will the boosting strategy induce communities that don't exist? To address this question, the authors include experiments with Erdos-Renyi graphs, which I view as a point in the authors' favor. Nevertheless, in Table 2, the ER graph ends up with extremely high modularity and low conductance for the quality function "ConsistentNO". This weighs against the method. How do you rule out spuriousness here? Boosting dense regions is a relatively easy problem. The challenge is how to avoid doing so where you shouldn't.</p>

<p>Detailed comments for the authors; justification of your overall rating</p>	<p>I'm also not convinced with the algorithmic approach. Even though the authors portray the work as boosting and bandit learning, the approach is very far from these theories. For example, boosting is not applicable because there is no weak classifier. In addition, the rewards of the bandit learning process vary at each iteration. I'm not aware of any result in the bandit learning literature that deals with arbitrarily varying rewards. As a result, I'm not sure whether the process is really learning from the underlying distributions or simply arbitrarily distorting the graph. It is not even clear that the algorithm will always converge, especially to a solution that makes sense (the convergence theorem they proved is for a different problem where the rewards are based on ground-truth). In fact, because the reward function changes at each iteration, I suspect that, given a bad start, i.e., bad initial samples, the algorithm would go astray and lead to a bogus unified graph.</p> <p>I believe that a strong contribution towards the solution of the proposed problem must address the above issues thoroughly and provide compelling evidence that noise is not being amplified. This methods and results in this paper, unfortunately, are not very reassuring. In addition, the problem is not new, but the author do not provide a comparison with previous approaches. Therefore, I recommend the rejection of the current manuscript.</p> <p>Some specific comments:</p> <ul style="list-style-type: none"> - The experimental set up where different edges are generated from the data is very nice, e.g., co-authorship and topic similarity in DBLP. - There is an underlying assumption that edges are independent. This is a simplification that could lead to bad starts as we fail to represent crucial inter-dependencies in the data, especially when the algorithm is cold. - The algorithm apparently learns and corrects for the edge weights ordering, e.g., for DBLP the order of weights are reversed by the algorithm. What does it mean to set up weights a priori? Does it matter at all? - Modularity and conductance are averages. I think that a finer-grained measures of "communitiness" would shed more insight on the results, which you were able to successfully do through pictures.
<p>List of typos, grammatical errors and/or concrete suggestions to improve presentation</p>	<ol style="list-style-type: none"> 1. What does it mean to have a NMI value for the ER graph? What is the ground truth, as there is no community structure? 2. Missing period between the words "structure" and "Both" in the third paragraph of Section 4.3 3. In Figure 3, bottom graph. What do the colors mean? 4. In Figure 6, what are the values of V, E, the number of layers and δ?